

THE
BOSTON MEDICAL AND SURGICAL JOURNAL.

NEW SERIES.]

THURSDAY, SEPTEMBER 3, 1868.

[VOL. II.—No. 5.]

Original Communications.

SECTION OF THE OPTIC NERVE AND
CILIARY NERVES, INSTEAD OF ENU-
CLEATION, FOR SYMPATHETIC OPH-
THALMIA.

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DR. RONDEAU, in a pamphlet published in 1866,* on sympathetic ophthalmia, says, in concluding:—"In studying the pathology of the secondary lesions which occur in an eye subsequent to injury of its fellow, in considering the intractable nature of the affection, its insidious and intermittent march, we notice the striking analogy between neuralgias and so-called sympathetic ophthalmia. No affection is so rebellious to medical treatment, and when in facial neuralgia all our therapeutic remedies are exhausted, we often have recourse with success to section of the nerve, and if this does not suffice, to excision of a piece of the nervous trunk. Not yet having clinical facts to rely on, I can but express the opinion that the same treatment is applicable to sympathetic ophthalmia. It is often difficult to make the patient comprehend, in an affection so slight in appearance, the danger of his situation and the necessity of removing the eye. He hesitates, and, waiting till the eye becomes enfeebled to the last degree, re-appears to confirm the surgeon's prognosis. When, finally, he decides upon an operation, the changes in the globe have often so far advanced as to insure but a limited advantage from enucleation of the injured eye, if he is not already condemned to incurable blindness. The first indication in this disease is to act as promptly as possible, in order to prevent the changes of secretion and structure consecutive upon reflex troubles of the circulation."

"Break the nervous chain necessary to the accomplishment of the changes which

constitute reflex action; divide the sensitive nerve, or destroy the ganglionic centre, or divide the recurrent nerves (electromotor), any one of these lesions, the breaking of the chain at a single point, is sufficient, and all re-action, all sympathy between the organs ceases."*

Dr. Rondeau then goes on to say:—"Nothing is easier than this operation, which I have several times practised in the amphitheatre. It consists in making a small opening through the conjunctiva at the upper and inner part, and introducing a slightly curved tenotomy knife, to be kept close to the globe. We divide, at the same cut, the ciliary nerves, the optic nerve and the central artery. The advantages and disadvantages of this operation are these: It is very simple, and as the patient has much less dread of it than enucleation of the globe, he is less reluctant to submit to it earlier. It serves the same purpose as enucleation in breaking the continuity of nervous tissue. Section of the optic nerve, it is true, destroys vision in the eye; but as in the majority of cases vision, if not lost, is greatly reduced, it is better at once to sacrifice the eye becoming useless, and which may endanger the other.

"In animals, this section of the nerve does not cause hæmorrhage to be dreaded; we only cut vessels of small calibre, the ciliary arteries and the central artery of the retina, the hæmorrhage ceases quickly, so that the blood does not exercise any injurious pressure on the nerves we have cut.

"If this section does not avert the progress of the reflex trouble in the sound eye, we must at once enucleate the affected one. Experience will decide on the value of this operation."

In the Memoirs of the Vienna Academy, 1864, Dr. B. Rosow gives two ophthalmoscopic pictures of the eye of a rabbit, before and after section of the optic nerve. From his experiments carried out in the Physiological Institute of the Vienna University, he draws the following deductions:

* Des Affections Oculaires Réflexes, et de l'Ophthalmie Sympathique. Paris. 1866.

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* Leçons sur le diagnostic et le traitement des principales formes de paralysie des membres inférieurs. Par Brown-Séquard.

1st. "Section of the optic nerve (in rabbits) when unaccompanied with accidents (disturbance of the retinal circulation, severe inflammatory reaction), does not cause, as formerly supposed, such rapid fatty degeneration of the fibrous layer of the retina. In one case, he found the largest part of the fibres perfect after one hundred and forty-two days. This is more than time enough for fatty degeneration to take place in other nerves separated from the central system. In another case, after one hundred and seventy-eight days, well-preserved nerve-fibres were found, although few in number. 2d. The causes which destroy the fibrous layer of the retina have no injurious effect on the other retinal elements; from which we may conclude that the vitality of these elements is entirely independent of that of the fibrous layer."

We mention these results of Dr. Rosow because they may throw some light on the question of the usefulness of section of the optic nerve in man. The Journal has lately noticed this as a new operation spoken of by Prof. Graefe as a remedy for subjective luminous sensations, and as he has changed his views as regards its applicability in sympathetic ophthalmia, we quote the following from the report of the Ophthalmic Congress at Paris last year:—"I have at times recommended the substitution of this operation for enucleation to counteract sympathetic ophthalmia, but experience and more mature judgment of the subject have soon shown its inapplicability. It is undoubtedly through the *ciliary* nerves that sympathetic ophthalmia is communicated, and section of the optic cannot therefore serve to cure it.

But there is another order of symptoms besides pain, and which in the class of special sensations is also dependent upon the optic nerve. I mean the subjective luminous sensations remaining after the loss of the eye, and accompanying certain forms of blindness; for instance, in detachment of the retina after irido-choroiditis, and when there is calcareous deposit within the globe, we often have subjective sensations very distressing to the patient. They have spectral coruscations (photopsia) tormenting them day and night, and we even find these followed by cerebral hallucinations, due to irritation of the brain from communication with the optic nerve.

Section of the optic nerve seems indicated under other circumstances also than these photopsic phenomena observed with the blind. I mean where there are intra-ocular tumors. It is well known that the

trunk of the optic nerve is particularly liable to propagate degeneration (sarcomatous or carcinomatous) to the parts outside the eye. Microscopic examination of the cut surface of the nerve after enucleation often shows us that it is already here affected, although the interior of the globe may be but partially filled with the tumor. In such a case the insufficiency of the extirpation will be apparent, and we shall soon have a return or the development of the tumor from the orbital part of the diseased nerve. If we are prompt enough in ascertaining that the cut surface of the nerve is diseased, the free end of the nerve must be cut off immediately after enucleation. This, however, is very difficult to do, and we cannot reach the remaining portion of the nerve in the orbit without extending our dissection in a dangerous manner. It is, therefore, more prudent, in cases of intra-ocular glioma, sarcoma and carcinoma, to first divide the optic nerve beneath the conjunctiva, and this as far back as possible, then proceed to enucleate. We shall thus have some third of an inch of the nerve with the globe, which we shall be glad of having effected if the cut surface proves healthy, whilst nearer the globe the nerve is found diseased.

Method of Operating.—We penetrate the orbit, or rather the celluloso-fatty tissue, by puncturing the cul-de-sac of the conjunctiva at its outer side, whilst the globe is drawn forcibly forward. The instrument used is a very strong tenotomy knife, moderately curved, which must follow the external wall of the orbit till the point is far enough advanced to cover the optic nerve. The latter being stretched by the position given to the globe, is readily presented to the concave cutting edge of the knife, and divided according to the rules for subcutaneous section. The distance of the section from the globe may be greater or less, as we choose. It is more readily made about one fourth of an inch behind the sclerotic, but can be carried within a few lines of the optic foramen.

I performed this operation with success in a case where blindness had lasted fourteen years. I observed, at the time, the difference between the character of the *retinal* hallucinations, as we notice them, and the *cerebral* hallucinations properly so called. In the first, simple figures are seen, geometric, so to speak; in the latter appear all the products of the imagination and the memory."

It seems pretty definitely settled that sympathetic irritation is conveyed by the

ciliary nerves and not by the optic; section of the latter, therefore, would be confined to such cases as Graefe speaks of. The knife sent me by L  r, of Paris, for this purpose, has a shaft about two inches long, and the blade, but slightly curved, five sixths of an inch long. Its use, of course, necessitates perfect familiarity with the anatomy of the orbit and the relations of the soft to the hard parts.

Prof. Graefe, having convinced himself of the action of the ciliary nerves in conveying sympathetic irritation, proposed, in 1866, in the *Archiv f  r Ophthalmologie*, the section of these nerves locally, where pain was produced by pressure on the sclerotic over them. The operation was, however, practised for the first time in May, 1866, by Dr. Ed. Meyer, who translated into French Graefe's "Clinical Ophthalmology." In the September number of the *Annales d'Oculistique*, Dr. Meyer reports three cases operated on, the method of doing which latter I translate from this piece. "Having determined on the place, painful to the touch, where section of the ciliary nerves is to be performed, we lift up and cut a fold of the conjunctiva near the edge of the cornea, just as in the operation for strabismus. Then, with blunt-pointed scissors, we separate, to the extent determined on in the plan of our operation, the cellular tissue uniting the conjunctiva and sclerotic. The eye is now fixed by a strabismus hook passed under the nearest rectus muscle; we thereby also determine the place of insertion of the tendon of this muscle. Holding the hook in the left hand, we puncture with Graefe's narrow knife the sclerotic in the ciliary region, obliquely to its surface, avoiding the crystalline lens. The counter-puncture is made so that when the cut is finished we shall have a linear wound parallel to the border of the cornea, in which the vitreous immediately presents. We then carefully remove the hook, and turn back the conjunctiva towards the cornea. Suture of the conjunctiva has not seemed to us of any real advantage; there is, however, no difficulty in applying it. The edges of the sclerotic wound remain several days ununited, and in one case we were obliged to cut off the vitreous on the third day, and apply the compressive bandage more than a week. There is very moderate reaction after the operation, and only rest needed, unless pain or sleeplessness call for subcutaneous injection of morphine on the temple, and the compressive bandage. Of three eyes operated on in this manner, one which had already commenced to atro-

phy before the operation continued to do so completely afterwards; the other two retained their normal shape."

Time must prove whether this section of some of the ciliary nerves, namely, those shown to be implicated by pain upon pressure over them, will not supersede enucleation of the globe otherwise indicated to prevent insidious sympathetic ophthalmia. Being forced to wear an artificial eye entails considerable expense upon the patient, and I have too often seen constant trouble from the best made and most perfect fitting one. I therefore look with much interest for the results of the operation as now practised at home and abroad. When, however, this is unsuccessful in preventing or subduing sympathetic ophthalmia, we have still left the certain remedy of enucleation of the globe, as I have fully shown in a late paper before the Massachusetts Medical Society.

TUBERCULOSIS AND PULMONARY PHTHISIS. A CRITICAL REVIEW.

(Concluded from page 52.)

THE doctrine of the caseous pneumonia of epithelial phthisis, regarded as the distinct disease of tuberculosis, is not, as is seen, solidly founded upon pathological anatomy, which leaves, as M. Ch. Bouchard says, doubts impossible to remove.

Does this doctrine find a more solid support from clinical experience? We shall now examine this question.

Since I can use the right of the critic, I shall take the liberty of immediately bringing up a contradiction which escaped Niemeyer. In most cases, he says, the three forms of phthisis can be distinguished from each other *almost with certitude*; and a few lines further on, doubtless regretting having said so much, he adds, we shall try to draw the picture of the three principal forms of pulmonary phthisis. M. Bouchard considers the differential diagnosis of simple caseous pneumonia and of caseous pneumonia complicated with tuberculosis as surrounded with almost insurmountable difficulties.

Whatever it may be, let us, however, see upon what signs Niemeyer bases a diagnosis.

1. Great frequency of the respiratory movements without augmentation of the souffle, and dulness, in a patient who has had for some time the signs of pulmonary induration, may cause us to suspect that a tuberculosis has arisen to complicate an already existing phthisis.

2. Pains in the chest and through the shoulders more frequently accompany the pneumonic processes than the tuberculous.

3. If cough and expectoration have preceded fever and loss of flesh, it is a pneumonic process we have to contend with; if the contrary, then we should regard it as a tuberculous phthisis.

4. An intimate mingling of blood with the mucous and muco-purulent sputa constitutes a sign of the pneumonic process.

5. A persistent cough, continuing a long time, with scanty expectoration, is a sign of tuberculosis.

6. A hoarse cough is one of the particular signs of tubercular phthisis, or of a complication by tuberculosis of a pulmonary phthisis, due primarily to destructive inflammatory processes.

7. The type of the fever in simple pneumonic process is remittent; the fever tends towards the continued type in tubercular phthisis and in phthisis complicated with tuberculosis. In other words, in simple caseous pneumonia the temperature in the morning varies little from the normal standard, and rises in the night to 1° , $1^{\circ}.5$ to 2° , while in tubercular phthisis the temperature keeps up above the normal state, sometimes reaching 41° , and does not fall sensibly towards morning. (*Recherches sur la température dans la phthisie pulmonaire, par Sidney Ringer, Archives de Méd., 1866.*)

8. In tuberculosis, the loss of flesh and the impoverishment of the blood are much more rapid than in caseous phthisis.

9. A shrill sound, not flat, tympanic, limited to the summit and coinciding with a diminution in the fulness of the respiratory movements, should lead us to suppose a tuberculosis.

10. Cavernous sounds, finally, are not produced in pure tuberculous consumptions, and most large caverns *should be considered* as due to the breaking down of the tissues affected with caseous infiltration.

When we consider that, according to Niemeyer, the coexistence of tubercles and inflammatory caseous masses is very frequent, that granulations, as he also allows, may be developed in the lungs in so latent a manner that they cannot be diagnosed, where is the physician who, in similar conditions, with such slight differential symptoms, would dare to give a diagnosis, that is, would positively state that a patient is or is not affected with tuberculosis?

• If, under a like condition, a vain scientific curiosity only is to be satisfied, I shall not insist upon it, but if we accept the

views of the Professor of Tübingen, that upon the precision in diagnosis evidently depends the gravity of the prognosis, then it is of the greatest importance that the problem should be solved. If the patient is attacked with a simple caseous pneumonia, it is not impossible to cure it, while if it is a tuberculosis, either primary or consecutive, then treatment is positively of no avail. Therefore, I repeat, the consequences depending upon the diagnosis are so serious that to establish it we require more marked and better defined signs than those indicated by Niemeyer, which he is far from considering as being always sufficient.

To distinguish tuberculosis from caseous phthisis, it has been thought that a criterion has also been found in the duration of the disease, by laying down as a principle that tuberculization is always acute. "It destroys," says M. Bouchard, "rapidly, like acute diseases; when it is prolonged, the new attacks reproduce acute manifestations, and if its total duration attains six or seven months, there is nothing established which shows that it can pursue a slow or gradual course like common consumption."

We can, in fact, regarding it from a certain point of view, say there is no chronic tuberculization, for in rapid consumption as in slow consumption, each new irruption of granulations produces acute symptoms, which are more or less intense, destroying the patient or gradually wearing him out.

In both cases the phenomena are the same; they only differ in their degree of intensity.

To demonstrate the relation between the two diseases, do we not see, every day, patients who present, as the effect of an irruption of granulations, acute symptoms, which would seem enough to carry them off, but who apparently recover, and the disease pursues its fatal course, with all the alluring signs of a chronic affection.

Such was precisely the case of the patient of M. Colin, to whom M. Bouchard alludes, and whom he considers as having had primarily an hæmoptysis and pneumonia; a patient in whom all the symptoms of an acute phthisis were rapidly developed, and which ended by becoming chronic.

Admitting that any form of pneumonia (fibrinous or catarrhal) can terminate, under certain conditions, by caseous infiltration, that a bronchial catarrh can produce catarrhal pneumonia by the propagation of the disease to the alveoli, and pulmonary phthisis by caseous transformation and the consecutive breaking down of the inflam-

matory products, Niemeyer naturally returns to the theory of a *neglected cold*, which we thought was buried forever.

In this, however, he approaches the opinions of Broussais, who also admits that all pulmonary inflammation can degenerate into phthisis, but into true tuberculous phthisis, tubercle being, according to him, the most ordinary result of these inflammations when they persist beyond their habitual term. (*Histoire des phlegmasies chroniques*, t. iii., p. 216.)

The first thing, the interpretation of which immediately embarrasses the author, is the persistence of a bronchial catarrh, limited to the summits of the lungs without the previous existence of granulations. According to him, this catarrh is not, as is generally believed, the evident sign of a commencing pulmonary tuberculosis, but very certainly a sign which announces that the patient is threatened with phthisis. As death does not ordinarily occur at this period of the disease, Niemeyer can hardly call pathological anatomy to his aid to justify his assertion, and clinical experience does not offer any greater resources.

If tubercle can exist in the latent state, and if bronchitis, when present, is only the indication of it, and M. Villemin agreeing upon this point with Laennec, Louis, Andral and Fournet, is there not, then, reason for regarding the bronchitis which is then induced by the slightest causes, as the effect in reality of the tuberculous neoplasm?

Again, in persons affected with bronchitis the signs of phthisis are concealed by those of inflammation of the bronchi, and only appear when the diminution of the local symptoms of bronchitis reveals those of tubercularization, which are on that account none the less primitive.

The same remarks are applicable to chronic inflammations of the lungs and pleura, and according as these complications are predominant, they control the pathological condition, even completely conceal, at times, the presence of tubercles, and cause the primordial lesion, of which they are only the effect, to pass unperceived. It is this form of disease which Broussais proposed to call tuberculous pneumonia, in contradistinction to pneumonic phthisis, that is to say, that in which tubercles manifestly preceded the pulmonary inflammation.

It does not follow from this that tubercularization cannot be developed in a person some time affected with bronchitis, pneumonia or simple pleurisy; but it seems to me necessary to recognize some relation of cause and effect between these facts. We

may say, with M. Pidoux, that tubercular and catarrhal diseases pursue two parallel lines, and however prolonged they may be supposed to be, these two lines may never meet. When we treat of the nature of tubercle and tuberculosis, we shall have occasion to speak of this point more in detail.

Pulmonary tubercularization, as all admit, progresses by successive irruptions of granulations, which excite about them more or less severe inflammation. This normal course of the disease, it appears to me, satisfactorily explains the various phenomena which Niemeyer, in common with all observers, has remarked in the course of pulmonary phthisis. After each irruption of granulations, there are present for some time acute symptoms, the progress of the disease is accelerated, then comes a period of comparative calm up to the breaking out of new granulations; and I find it entirely useless, on this account, to say with Lebert, that a patient can be consumptive to-day, may not be so in three months, and may be so again in six months.

Primitive tuberculosis, at the beginning of phthisis, being the exception, according to Niemeyer, in order to explain the frequency of hæmoptysis at this period of the disease, he admits then that the hæmorrhage and the phthisis come *simply* from a common source, that is to say, from a double predisposition of the patient to hæmorrhage and to phthisis. Hæmorrhage, according to the Professor of Tübingen, can precede phthisis, and induce chronic inflammations of the lungs, which are followed by destruction of the latter. If I did not hesitate to surpass the limits of a critical review, which should always be courteous, I should retort upon Niemeyer the reproach he addresses to Laennec of having made an unfortunate application of the principle, "*Post hoc, ergo propter hoc.*"

In most cases, an hæmoptysis is followed by a more or less severe irritation of the lung or pleura; this fact is incontestable, and can be easily explained if we admit the doctrine of Laennec—this aggravation is nothing more than the index of a new irruption. Hæmoptysis, says M. Villemin, can only be the result of pulmonary congestion at the commencement of the evolution of the process, and of the obliteration of the vessels in the neighborhood of the nodules, whence increase of intra-vascular pressure, then rupture of capillaries, and bloody extravasation. Is not this interpretation of the facts, which clinical experience, moreover, confirms, infinitely

more satisfactory than the hæmorrhagic diathesis invoked by Niemeyer?

It seems to me that a definite conclusion may be drawn from this long discussion:—that pathological anatomy does not furnish proofs sufficient to establish the independence of caseous pneumonia and tuberculosis; that clinical experience is even still more powerless in demonstrating this duality of pulmonary phthisis.

If we admit that tuberculous granulation preëxists, that it is always the primordial lesion, then all is joined together, all is united. If, on the contrary, this great fact is not admitted, everything becomes confused, uncertain, and as there can be but one true theory, the multiplicity of doctrines which we have seen successively arise, can only have the effect of exciting doubt in the minds of those who would believe.

For instance, catarrhal phthisis—caseous pneumonia of Niemeyer, caseous phthisis of M. Coursières, epithelial phthisis of M. Feltz and of M. Chatain, tuberculiform phthisis of M. Hirtz, tuberculide of M. Sorel, disseminated chronic pneumonia of Lebert, &c.—this phthisis, I say, which does not want for names, is even to the best informed nothing else than demonstrated.

For my part, I am convinced that the doctrine of Laennec will come out triumphant from all attacks, that modern researches will only give to it a still more brilliant confirmation, and, finally, that M. Villemain was inspired when he proclaimed anew that every pulmonary consumption was the result of the evolution of tubercles.

But there is a disease also characterized anatomically by granulations disseminated in different organs, and especially in the serous membranes, which, from its peculiar symptoms and its generally rapid course, seems at first to merit a distinct place in the nosological table—I speak of acute tubercularization.

M. Empis, adopting entirely this view, has made a special disease of it, and has called it granule.

Other authors, returning to the views of Bayle, have also desired to make a particular disease of that phthisis which is characterized by the rapid development of miliary granulations in the lung (phthisis granulæuse miliare).

But if we closely examine these facts, we shall see that these distinctions have no foundation.

One thing seems to me to be immediately established, that it is not possible to consider as two distinct diseases granular pul-

monary phthisis and acute tubercularization. It seems to me that this granular phthisis rather serves as a uniting link between chronic phthisis and acute tubercularization, and for two reasons: first, because it is extremely rare not to meet granulations in other organs at the same time that they are found in the lungs; and again, because if the pulmonary disease has continued for some time, we find at the autopsy that a certain number of gray granulations have already undergone fatty transformation.

M. Colin has very clearly indicated this latter fact in his clinical studies, and the third observation of acute tubercularization which he reports can leave no doubt upon the subject.

But when the granulations are general, when they invade the meninges, the pleura, the peritoneum, the pericardium, &c., the disease no longer presents the same aspect.

Granulation, always identical, produces different effects according to the organ in which they are developed, and there is nothing in this fact which can justify a fundamental distinction.

Age, in particular, exercises very marked influence upon the degree of generalization of granulations, and Lebert observed with reason that tubercles are so much the more abundant and are disseminated through a greater number of organs, the younger the person and the nearer he is to infancy.

When at the autopsy no softened granulations are found, it is, as M. Peter remarks, because the rapid progress of the disease has not allowed the fatty transformation to take place.

Consequently, according as this or that organ is the seat of the disease, according to the number of organs affected, according to the abundance of the granular irruption, according, finally, to the conditions special to each individual, the symptoms, the progress, the termination even of the disease differs, but the anatomical lesion is always the same, and continues to characterize the affection.

Examples of diseases identical in their nature and different in their clinical aspect, are frequent in pathology.

The lobar pneumonia of the infant, the acute pneumonia in the adult, and the insidious pneumonia of the aged, although very different when regarded from a clinical point of view, are nevertheless one and the same disease.

A varioloid and a confluent variola, differing as they do in seriousness, are still the result of one and the same cause. An acute cancer does not differ as to its nature

from a cancer which pursues slowly and fatally its destructive course.

I do not intend to insist upon this point; it is only necessary to present the question in such terms, in order to have a unanimous response from all clinical teachers.

Not content with complicating facts, names have been completely mystified. The granule of Empis is the tuberculation of Virchow; the tuberculation of Empis is the caseous pneumonia of the Germans. Galloping consumption was admitted as designating the common, vulgar phthisis, having a rapid course; and acute consumption, as designating pulmonary granular phthisis, until Trousseau, and after him M. Jaccoud, in his notes to Graves's Clinical Lectures, overturned completely this nomenclature.

To avoid all confusion, it seems to me that it would be much simpler to admit a pulmonary tuberculation, or, if you please, a *chronic pneumophymia* (common, vulgar consumption), a *sub-acute pneumophymia* (common phthisis running a rapid course), and an acute phthisis (granular phthisis), reserving the expression of *acute tuberculation* for those cases where the granulations are more or less general.

In resuming: the doctrine of the unity of tubercular diseases is not rendered hypothetical by clinical experience nor by pathological anatomy, and in terminating I shall repeat, with the authors of the Compendium, that the only true manner of philosophically regarding pulmonary phthisis is that which consists in considering it as the localization in the lung of a general affection called *tuberculation*, which can be localized in different organs.

I have designedly left completely untouched the question as to the nature of tubercle and tuberculosis. This will be the subject of a second article.

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Reports of Medical Societies.

MIDDLESEX NORTH DISTRICT MEDICAL SOCIETY.
JOHN H. GILMAN, M.D., SECRETARY.

A QUARTERLY meeting of the Middlesex North District Medical Society was held at their rooms in Lowell, July 29th, 1868, the President, Dr. Jewett, in the Chair.

The records of the preceding meeting were read by the Secretary and accepted.

Dr. Allen, of Lowell, read a paper on *Criminal Abortion*. He proceeded to show the general prevalence of the practice in New England, particularly in Massachusetts, where it is fearfully common among the native population. It is practised to some extent in other parts of the country, but not to the same degree as in New England. He discussed at considerable length the effect of this pernicious custom upon the health of our females, attributing to it the commonness of the so-called "female diseases" among our women—such as miscarriage, prolapsus uteri, leucorrhœa, sterility, amenorrhœa, menorrhagia, and a whole train of nervous disorders. He said the offspring of such mothers make feeble men

and women, and that the result of this evil practice in decreasing and devitalizing our native population is already apparent. In conclusion, he referred to the action of the medical societies in the different States of the Union where the vice prevails to any extent, showing that it had arrested their attention and that they were making proper efforts to suppress it.

The President said the subject was now open for discussion, but at the request of Dr. Allen it was deferred until the next meeting.

Dr. Bass, of Lowell, reported a case of severe *Puerperal Convulsions*. The labor terminated at 6 o'clock in the afternoon. At 8 o'clock in the evening, the patient was attacked with severe convulsions; he at once administered chloroform, remained with her all night, and kept her under its influence until 10 o'clock, A.M., the next day, when he allowed her to recover from its control. She remained free of convulsions until 8 o'clock the following evening, when she had another attack. Chloroform was resorted to again, and she was kept under its influence and that of ether until 10 o'clock, A.M., the next day. After this the patient did well. During the thirty-eight hours' time she was twenty-eight hours under the influence of chloroform, inhaling eight ounces of chloroform and about the same quantity of ether. In this case, it was found that chloroform controlled the convulsions better than ether.

Dr. Burnham, of Lowell, read a paper on *Chloroform, its advantages over Ether as an Anæsthetic*. He showed that chloroform is safer and is attended with none of the baneful after results which often follow the use of ether, such as insanity, epilepsy, convulsions, &c. He cited cases in his own practice which seemed to fully substantiate his assertion. He condemned the use of a mixture of chloroform and ether, and the alternate administration of the two anæsthetics as being fraught with more danger than either alone. He said the disadvantages of ether were:—1st, that it causes violent struggling, often requiring three or four strong men to hold a delicate female; 2d, that it produces a copious secretion of mucus from the lungs and bronchiæ, rendering it necessary to wait for the patient to free his lungs and thereby avoid impending death from asphyxia; 3d, that it is a powerful stimulant and causes congestion of the brain, which is liable to eventuate in apoplexia or paralysis; 4th, that it requires a much longer time to produce anæsthesia, and the

patient's sleep is more disturbed and much less tranquil than that occasioned by chloroform; and, finally, that it gives origin to subsequent attacks of insanity, epilepsy and other spasmodic affections. Chloroform, he said, is free from the above objections, and, if properly administered, is devoid of danger. His method of giving chloroform is to place a folded linen handkerchief in the bottom of a common bowl, pour upon it a fluid drachm of the anæsthetic, invert the bowl over the patient's face, keeping the handkerchief away from the nose and mouth by means of two fingers inserted within the bowl. The patient should be brought rapidly under the influence of chloroform, and the bowl should not be taken from the face until anæsthesia is produced, except to renew the chloroform; generally from one to three fluid drachms will be required to bring about anæsthesia. This state may be known by the snoring of the patient as in natural sleep; then the bowl should be taken *entirely* away from the patient's face, and the proposed operation commenced. It is more necessary to watch the respiration of the patient than his pulse, and to be very careful and not allow him to inhale enough to bring on the stertorous breathing of apoplexia instead of the snoring of natural sleep. None but a physician should be allowed to administer chloroform for any protracted operation, and he should know how to give it properly. Never trust a student with the administration of it, for he will become interested in the operation, neglect his duty, and allow the patient to become fatally narcotized.

Dr. Dow, of Westford, stated that while in the Western Army during the late war, he had given chloroform and seen it given in numerous instances, but had never seen any ill effects attending its use. He said the Western surgeons had a theory that Western men bore it better than Eastern.

Dr. Spalding, of Lowell, said he was in the habit of giving chloroform and ether alternately in the same case.

Dr. M. G. Parker, of Lowell, said he ordinarily used a mixture of one part chloroform and two parts ether.

Dr. Smith, of Lowell, thought that it would be safer to give chloroform and ether in the atomized state, as the proper intermixture of air would thus be insured.

Dr. Bass, of Lowell, stated that he considered chloroform equally as safe as ether and greatly to be preferred in practice. The violent struggling which ether so often

occasions is a great objection to its use, necessitating the presence of several assistants to keep the patient in subjection.

Dr. Gilman, of Lowell, remarked that he had generally used ether alone, or a mixture of it with chloroform, and that he had found that it was best not to use much constraining force in those cases that come under the influence of ether with difficulty, for the violence of their struggles is almost always increased by any efforts made to restrain them. Most refractory cases will become calm if the ether is taken away a few moments, and on resuming it will take it kindly and pass under its influence without further trouble. He mentioned a case which seemed to show that a continued use of ether will cause insanity. While on duty at a General Hospital in Washington, he made the acquaintance of a surgeon who had acquired the habit of inhaling ether for its exhilarating effect, and who finally became insane, and was taken to an asylum.

Dr. Burnham said, in answer to a question, that there were patients who, on account of idiosyncrasy or disease, will bear ether who will not chloroform, and *vice versa*, and there were some cases in which neither should be used, but for the generality of cases chloroform is to be preferred to all known anæsthetics.

Bibliographical Notices.

* *Observations on some of the Affections classed as Nervous Deafness.* By JAS. HINTON. Svo. Pp. 38.

Mr. HINTON begins his paper with a description of three cases of sudden deafness from apparently slight causes, in which, however, on further inquiry, it was found that some time previous to the appearance of the deafness each of the patients had received a concussion, which, he suggests, jarred the nerve and made it extremely liable to be affected by slight causes.

Recapitulating the pathological changes found on dissection of the nervous structures of the ear by Toynbee, Voltolini and himself, he considers the most important of these to be "the frequency of vascular congestion, and the extreme degree it reaches without the existence of any symptoms which have commanded attention." Debility, nervous or other, he does not believe to be a cause of nervous deafness, and cites a case in which at the time of greatest prostration

the hearing had decidedly improved; the case is also interesting as the patient himself distinguished his nervous deafness from a catarrhal deafness which supervened when he caught cold.

The signs by which nervous deafness may be inferred are given as follows:

1. History; besides blows on the head and loud noises, fever, sunstroke, mumps, diphtheria, parturition, residence in India, convulsions in childhood are the origin of many cases, but it is suggested that many of these may have been the result of the ear disease.

2. Peculiarities of hearing; worse on excitement, fatigue or depression; watch heard better than the voice, and also great diminution of the hearing on inflating the tympanum.

3. Degree of deafness; if excessive cannot depend on imperfect conduction.

4. Results with the tuning fork; if well heard through the bones of the head, the deafness is not likely to be nervous. Its greatest use, however, is in enabling us to judge of the power of conduction in the membrana tympani and ossicula; if the conducting media are healthy, the sound is increased on closing the meatus; if they are impermeable to vibrations, no effect is produced. Two cases, one of cerumen and one of closure of the Eustachian tube, are given to illustrate this point.

5. Aspect and manner of the patient, which are described as most commonly a "nervous unrestful manner, and a set, yet fluctuating expression of countenance."

Mr. H. believes that in a majority of cases of nervous deafness the tympanum is also diseased, and that the source of the nerve affection lies in the tympanic disease, caused in three ways:

1. By the propagation of a functional irritation.

2. By pressure exerted upon the labyrinth through the ossicula.

3. By changes in the labyrinth itself through disordered circulation, consequent on inflammation of the tympanum.

The opinion is then advanced and discussed at considerable length, that ear-ache in children may cause nervous deafness in after life; it is not, however, affirmed to be a direct cause, but "it appears that certain kinds of inflammatory affections of the tympanum in childhood so far involve the adjacent structures of the labyrinth as to predispose them to a loss of function from causes that would not otherwise lead to that result." Numerous cases are given in illustration.

It is also suggested that in many of those cases of deafness resulting from cerebral disease, it would be found rather that disease of the internal ear had induced the cerebral symptoms. In speaking of a case of total deafness following cerebral disease, in which all the visible parts were found to be normal, the author says: "It is natural to doubt whether an affection of the brain and its membranes, so severe, could have passed off so entirely, leaving no trace except on the auditory nerve: it is more reasonable to suspect an inflammation, possibly beginning in the tympanum, and by extension inwards involving the vestibule and cochlea."

The occasional cases where closure of the meatus diminishes the sound of the tuning-fork are then discussed. Increased pressure is often the cause of impaired vision, and from analogy may be supposed to be a cause of impaired hearing, as "the relations of the optic and auditory nerves to fluid pressure are strikingly parallel." "And if in any case deafness were dependent on increased pressure of fluid in the labyrinth, it would be expected that whatever tended, even in a slight degree, to add to that pressure, would tend also still farther to diminish hearing."

There is, then, a faint *a priori* argument that when closure of the meatus (which must tend, however slightly, to transmit pressure to the stapes and fenestra rotunda) diminishes bone-conducted sound, the labyrinthine fluid is in excess; that is, supposing the meatus and tympanum free." Such cases he considers as probably cases of glaucoma of the ear. The deafness gradually comes on without cause; the visible parts are healthy; often there are recurrent attacks of pain in one or both ears, often a history of neuralgia; tinnitus is always present. A case of this nature, improved by treatment, is reported.

J. O. G.

THE VAPORARIUM IN PHTHISIS.—We are indebted to Troussseau for the introduction of this means of treating phthisis. It is simply a chamber filled with the vapor of water, medicated or not, in which the patient resides for a longer or shorter time. It is, in reality, the substitution of a warm and moist climate for the ordinary climate of the locality. Several cases of phthisis are reported as having been cured by it. After all, however, the sugar-house treatment, as employed many years ago in New Orleans, is essentially the same, and takes precedence as to date of application.—*Pacific Medical and Surgical Journal*.

Medical and Surgical Journal.

BOSTON: THURSDAY, SEPTEMBER 3, 1868.

HOW TO STUDY MEDICINE.—No. III.

PATHOLOGY AND PHYSIOLOGY.—A PLEA FOR AUTOPSIES AND FOR VIVISECTIONS.—The student having mastered, or at least acquired a fair knowledge of anatomy and chemistry during the first year of his pupillage, has next presented to him two other practicable, demonstrable and most important branches, viz., Physiology and Pathology. By the first, we study the living functions; by the second, we determine organic changes after death. It needs no words to prove that both these branches should be taught practically, rather than didactically; over the subject of experiment or the dead body, rather than in the formal lecture. Here, again, the systematic lecture course is useful mainly in recording the history of these sciences, and not what they now are. We want to know the experience and results obtained by other observers, in order to verify our own. A very moderate amount of book knowledge should, therefore, suffice to introduce the student to the physiological laboratory and to the autopsy room. Once there, he should be taught to work with his own hands, see with his own eyes and make his own deductions. In this way, and in this only, will the knowledge of these branches make any lasting impression, or tend to any practical result.

Were it possible to introduce into this de-centralized and ungoverned country the simple, severe and straight-forward scientific edicts of a monarchy like Austria or Prussia, we might have some prospect of benefiting by the numerous cases of ill-recognized disease which pass out of our hands and knowledge after death. If all deaths in our hospitals and public institutions were invariably followed by autopsies, enforced by law, medical science would have one great obstacle to its advancement removed. This we fear can never be accomplished among our heterogenous population and under our changeable government. What precious opportunities are

thus thrown away as a tribute to superstition and mistaken reverence for the dead, every physician can testify. When we reflect that morbid pathology is one of those very few highways of science which lead straight on to certain results and to certain and most useful knowledge, we cannot but regret that so much should be lost for so little; so much good to our fellow-men for so little use to a narrow circle of ignorant and prejudiced acquaintances and friends. The good to be gained from autopsies is often inestimable; the evil done, as inestimable, in an infinitesimal ratio.

While, however, things are as they are—and in our day, at least, they will be no different—while autopsies are so rare and so uncertain, it should be the duty of every medical man, not only to make all he can, but to see that his followers, the students, are all well grounded in the ways of making them, and habituated in comparing the results of what they see. The autopsy room, next to the dissecting room, should be the student's constant resort. Here he may acquire more in one hour than in a day of lectures. He should be encouraged to visit it as soon as he has acquired enough knowledge of anatomy to enable him to estimate and to discriminate what he sees; and his attendance at autopsies should be obligatory throughout his whole three years' study.

If we pass now to Physiology we come to a department of our studies where the crucial test of experiment is the only recognized way to learn. Without ardent observers to experiment on the lower animals, as well as man, we should have, now, no Physiology, save that limited amount which can be deduced from watching morbid processes in disease; from judging of what healthy functions are by their loss and absence in sickness. To do away with the practical physiology of the last twenty-five years, would carry us back, relatively, a century in sound knowledge and in wise practice. When we think of the vast field of unexplored knowledge, of how little we know positively in medicine, and how much of that little has come through studying physiology by experiment, we cannot but be amazed at the mistaken philanthropy which decries vivisections as useless and cruel.

We hold that, as we have the right and the necessity to use the lower animals for food, so we have the right to use them for knowledge. We must bear in mind, and we may justly imitate, that great scheme of Nature which decrees that thousands shall die that one may live, and that a myriad of germs shall be initiated and fail of maturity, in order that one may be perfected. The spores of plants, the spawn of fishes, the spermatozoa of man, illustrate this law. Every class and every species preys upon its inferior class, and lives upon their loss. Surely the same reasoning may hold true in that far higher use to which man can put the lower classes, in order to learn himself, to study and utilize his functions, to save suffering, to apply treatment and to avert disease! Cruelty is a relative term. Were it necessary to destroy a swarm of insects in order to give one human being anæsthesia in pain, should we hesitate? Useless repetitions, idle experiments, unnecessary cruelty, we certainly deprecate. But we as certainly believe that ends justify means; that we have a right to employ our superior brain-power in finding out everything useful, even at the expense of our inferiors. And when we consider how much is to be gained for humanity, even the so-called humane must hesitate before they reject experimental physiology.

For first, this is the only path to a knowledge of functional life. All else is empirical and uncertain. The field before us resembles some great continent, like the *terra incognita* of the ancients, whose shores and coasts are partially explored, but whose vast interior is wholly unknown. And as long as it is unknown, imagination peoples it with monsters which disturb the fancy, and with superstitions which pass down from age to age, rule our art with a pure empiricism, and reduce the practice of medicine to a nurse's dogma, or an old woman's remedy. Even worse than this—in our anxiety to peer into this unexplored region of life, other superstitions, as spiritualism and *clairvoyance*, are invoked to lend their aid to charlatans and knaves. As knowledge advances, error flees. Were the truths of physiology known

and applied to therapeutics, quackery would diminish, and perhaps eventually cease.

By experimental physiology we may learn two great series of facts. First, the laws of functional life; as, for instance, digestion, circulation, &c.; and also the changes produced in different parts by different conditions. To instance one only. It is still unsettled whether anæmia or hyperæmia is the normal state of the brain in sleep—a question full of interest and importance to the nervous, wakeful invalid, as leading to the judicious use of remedies; a question, too, which might apparently be settled forever by means of careful experiments in trephining the lower animals.

Second; experimental physiology is the only way in which we can test the effect of drugs on the animal functions; and not only of drugs, but of articles of daily use, as food and drink. All other employment of remedies is purely empirical, and must remain so. Therapeutics without such investigations never can advance one step. We are as likely to do wrong as right; and resemble Molière's physician combatting disease, who lays about him with a club in the dark, uncertain whether he hits friend or foe.

PARALYSIS AFTER SCARLET FEVER.—The case was reported in the *Med. Times and Gazette*, by Dr. SHEPARD.

Jane W., aged five, said to have been quite well and at school on September 24, 1867, woke before daylight on the 25th, complaining of pain in the head. In the morning a red rash appeared on the chest and neck, and after the evening of that day the child was, and continued, absolutely speechless. Dr. Park and myself visited her next day. She was in bed with a sister, both with well marked rash of scarlet fever, but with little, if any, affection of the throat. Her sister recovered easily, but she remained still, after the disappearance of the rash, without speech or motion.

On October 22, she was admitted into the Infirmary. The following is her general history: She is the fourth of six children, the eldest of whom died after an attack of measles; the rest perfectly healthy. She is said to have had two "convulsion fits" about the age of twelve months while still sucking, and two others later, and subsequently "fainting fits whenever she was frightened;" nocturnal incontinence of

urine every now and then; no oxyurides ever noticed; no illness of any kind before this; said to be always quick and observant; parents healthy; never subject to fits.

She is a child of somewhat muddy looking complexion, with large dark eyes, long black eyelashes, and black hair.

On admission she was very thin, horribly dirty, and perfectly speechless and motionless. When taken out of bed, and placed on her feet, her legs sank under her, her arms and head fell forward, and hung listless. When asked to repeat a simple word, she only blew between her lips, and when told to copy two or three letters of the alphabet, or even mere lines, on a slate, she traced only vague scratches. In almost this same state, she continued for three weeks, the only noticeable fact being that she ate ravenously—a fact, I think, to be remarked almost always in cases of chorea and ordinary paralysis. After this period she seemed to improve rapidly. When supported, though her head hung forward, and her hands were stuck out, with all the fingers pointing, she walked with the action of a high stepping horse, throwing out her legs, and bringing the heel and the ball of the great toe sharply down on the floor, at the same time carefully keeping her eyes on every step she made. As she got stronger, her movements were more like those of a person suffering from ataxie locomotrice; the high action of the legs still continued; the extensor proprius pollicis of both feet was always contracted, never allowing the great toes to touch the floor. The fingers were no longer stretched out; she gradually became more intelligent; watching persons moving about the ward, smiling and nodding when spoken to. On November 7, when asked to repeat the word "egg," she breathed only the gutturals "ggg;" on the 9th she said, "goo," with a long blowing sound on the "g," and pursing up her lips, meaning to say, "good morning." All these symptoms vanished, one after the other; on Christmas day she got up and was dressed, for the first time, and is now (January, 1868) able to play about the ward with the other children, though her movements and speech are yet far from being normal.

Throughout her illness there was no heart mischief, neither was any albumen found in the urine; the paralysis was not more marked on one side than the other. Considering the nervous symptoms, like those often succeeding diphtheria, to be merely transitory, and following out the dictum of Celsus,—*"opportunum medica-*

mentum est opportune cibus datus"—we treated the patient with nothing else than the ordinary diet of the infirmary.

DISLOCATION OF THE KNEE.—The following case is reported in the *Lancet*, by JAMES CARMICHAEL, M.D.

The comparative rarity of dislocation of the knee seems to justify the record of the following case. Practically it is of interest on account of the recovery of perfect use of the joint.

On the 15th of last July, James W., in the employment of the North British Railway Company, while endeavoring by means of an iron crowbar to put the brake upon one of two trucks, which were being prepared to run down an incline into the goods steamer, unfortunately allowed the bar to get between his legs, whereby he was at once thrown down when the trucks got into motion. He was lifted aside, and I saw him shortly after. He lay flat on his back, with both limbs fully extended. He complained of great pain in the right leg and knee, down to the toes. On examining the limb, I found that in front the knee had lost its natural shape, there being a depression below the patella, while behind the head of the tibia could be felt pressing backwards in the popliteal space. There was no rotation of the leg whatever. Everything was quite rigid about the joint, and no crepitus could be felt. In order to effect reduction, while the thigh and pelvis were kept firm by an assistant, I grasped the leg, and made steady traction. After pulling for a little without reduction being effected, I was induced to flex the knee slightly, and, in doing so, I was speedily gratified by seeing the head of the tibia glide slowly forward over the condyles of the femur, into position, showing thus very clearly the extent and nature of the injury. The patient was afterwards placed in bed, and the limb maintained at perfect rest. Considerable ecchymosis occurred, as evidenced by great swelling and discoloration of the skin on the posterior part of the joint. The joint itself at first remained free from any effusion, but subsequently, when the swelling resulting from the bruise began to subside, a good deal of passive effusion occurred. By a continuance of rest, aided by friction and a bandage, this soon became quite absorbed, and by the 20th of August, about five weeks after the receipt of the injury, the patient could progress with the aid of a crutch. Now he walks, although slightly lame, with perfect freedom, and without any mechanical assistance.

CASE OF CONGENITAL IMPERFORATE RECTUM, SUCCESSFULLY TREATED WITHOUT A CUTTING INSTRUMENT. By J. D. B. STILLMAN, A.M., M.D.—In the year 1848, a case of congenital malformation of the rectum occurred in my practice, that was operated on by Dr. Willard Parker, of New York, with a bistoury. He succeeded in reaching the rectum above the obstruction, and discharging the contents, but the infant died soon after. I then thought that should another case like it occur to me, I would adopt another method. The uncertainty that must necessarily exist as to the extent of the malformation above the anal terminus, will always render the success of any operation by any method doubtful.

I was called to see a male child of C. Y—, at No. 363 Jessie street, on the 12th of April, 1864. The infant was four days old, and had not had an evacuation of the bowels, had worried much, and was now almost continuously vomiting. The point of a catheter could be carried through a well formed anus about three fourths of an inch, when its further progress was arrested. An examination with the finger confirmed the nature of the difficulty.

There was a perfectly formed cul de sac, the fundus of which was firm, and resisted all the force I deemed advisable to use. Its depth was not far from three fourths of an inch. I had now a case, as far as could be determined by *ante-mortem* examination, like the one fruitlessly operated upon by the knife twenty years ago.

What lay beyond the membrane that resisted the further progress of the finger, who could tell? Was it a simple diaphragm of a line or two in thickness? If so, I should be able to detect the distended sac above. Or was the rectum, though formed above the cul de sac, still adherent by its opposing surface through several inches of its course? Was it atrophied to a cord or rudiment of a gut? Were the two sections of rectum running parallel to each other, as in the cases reported by Amussat, Goddard and Curling? or was the rectum wholly wanting? These were conditions either of which was possible, but the determination of which was not possible by any means of diagnosis we possess.

Of the thirty-one cases tabulated by Mr. Curling in the forty-third volume of the "*Medico-Chirurgical Transactions*," in which the external portion terminated in a cul de sac, as in the present case, sixteen were successfully operated upon, so far as reaching the gut was concerned, and ten

were reported cured. Of this number two were dismissed, as the time that intervened between the operation and the date of the reports was too short to admit of satisfactory conclusions being drawn respecting them. All the other cases terminated fatally. The cause of death in all cases where the gut was reached was either from injuries inflicted by the knife or trocar upon contiguous tissues, or from the difficulty of maintaining an opening. These results are almost unavoidable when a cutting instrument is used, except in those cases where a protruding diaphragm gives a certain guide for the instrument. Where the gut is collapsed or attenuated, the cutting instrument can scarcely fail to miss its course and find its way through the cellular tissue around it. In such cases, even when the object was reached, it was found impossible to maintain the opening through the fistulous track. These considerations determined me to try a blunt-pointed instrument, trusting to the strength of the fibrous coat of the intestine to retain the instrument within its walls, if there were any; and, in case there were none, it could not fail to reach the pouch above as safely and as surely as a cutting instrument.

I accordingly prepared myself with a number of bougies, to meet any contingencies. I told the parents that I had but faint hopes of saving the child, but I thought it proper to afford the little chance that the operation could give. I first attempted the passage with a No. 12 English bougie; but after using as much force as I deemed safe for fear of transverse laceration of the gut, I next introduced a conical-pointed wax, of the size of about No. 6, and pushed it in the direction where I ought to find the upper portion of the rectum. This was carried up about two inches, or until resistance ceased. On withdrawing the instrument, only the color of the meconium on its point showed that I had accomplished the object. I then introduced the larger bougie, which had failed before, and carried it up in the track of the smaller one, and, upon withdrawing it, I had the satisfaction of seeing a discharge of about four ounces of meconium with flatus. The passage was further distended by larger instruments, until a free passage was afforded. No further treatment was found necessary. The vomiting ceased and the child rallied from a condition so low as to give no sign of pain during the operation. Some months afterward defecation was difficult, but not sufficiently so to warrant interference, and when three years old it entirely disappeared.—*California Medical Gazette.*

HOW TO TRAVEL FOR HEALTH.—The architect in England may consider that cedar might be the best wood for a certain purpose; but if he had to send to Lebanon for it, he might think the cost of carriage would counterbalance the particular suitableness of the wood, and he would seek another tree which would answer nearly as well. In like manner, Madeira may be the place for one recovering from inflamed lungs; but the transit thither may so injure the health that sea sickness will effect what the previous disease had spared. Again, the breadwinner of a family may require change of air; and if a locality is ordered that is too expensive for his means, the mental distress arising from doubts about the future position of himself and of his dear ones, may counterbalance the benefits which the climate ought to bring. We conceive, when change of air is required for the recruitment of health, that the doctor, instead of shaking his head ponderously and issuing, in a decided tone, the name of one locality, should go through a catechism something like the following: Have you fondness for one spot more than for another? No.—Well, what do you enjoy the most when you are well—do you like fishing, sketching, boating, yachting, mountain scenery, photography, or much society? Still farther, how much can you afford to spend over yourself? According to the replies to these questions the physician would recommend a quiet valley with a good trout stream, or a spot like the Betws-y-Coed, in Wales, where materials abound for the most ardent limner, or Beaumaris, where there are both boating and yachting; or Bowness, where there are boats in plenty, fish in abundance, society galore, and scenery the most lovely. The main requirements for an invalid who is recruiting health are animal or mental enjoyment, warmth, air without much exercise, and a good *cuisine*. The influence of change under these circumstances is very marked. I well remember my recovery from an attack of fever—too languid to care much whither I should go, my father decided upon sending me to Llangollen, and I went there with my mother and brother. As the carriage bowled along the level plains I was only conscious of fatigue; but as we entered amongst the mountains the sight of them was like a moral draught of champagne, and I became as excited as if I had drained a bottle of that wine. My brother had a kindred spirit, and we did not sleep until we had climbed to the summit of the nearest hill. Thence we saw

another in the distance which was higher, and that we scaled the day after; and my recovery was as absolute as it was sudden. Now as I feel morally certain that such a result would not have occurred had I been condemned to visit a place which to me would be as stupid as Bath, Harrogate, Cheltenham, or Brighton, so I would not recommend any one to visit a spot where there are not some means of gratifying his peculiar pleasures. — Dr. THOMAS INMAN, *London Medical Mirror*.

UREA AND URIC ACID.—By RICHARD GRIFFITH, jun., Ch. M.—I have long been of opinion that uric acid in the human system is the immediate result of a deficient supply of oxygen in the blood, and consequently, that the only rational treatment for removing it consists in such processes as favor its elimination, combined with an increased supply of oxygen to the blood, and a diminution of the supply of nitrogen, which principally enters the system in the form of animal food.

I will not now, however, enter into the best means of favoring the elimination of uric acid, as my principal object is to show that it arises from a *deficient* supply of oxygen or *excess* of nitrogen in the body, and will accordingly be found most frequently to afflict the carnivorous, and those leading a sedentary existence. Now for the proof— $C_{10}H_4N_4O_4$ are the chemical equivalents of uric acid, if 4 atoms of water= $4H_2O$, be added, we have $C_{10}H_8N_4O_{10}=C_4H_8N_4O_4+6(CO)=2$ atoms of urea ($C_2H_4N_2O_2$)+6 (co); if to this product be added 6 atoms of oxygen= C_4O_6 , we have 2 atoms of urea +6 carbonic acid (CO_2)=1 atom of uric acid+4 atoms of water+6 atoms of oxygen; thus showing that 6 atoms of oxygen, in addition to 1 atom of uric acid, are required to form 2 atoms of the soluble substance, urea, and to oxydate thoroughly the accompanying 6 atoms of carbon. I hope this formula, which I am not aware has ever been presented to the profession before, may reconvert Dr. Kelly to his former sound opinion, and influence medical treatment accordingly.—*Medical Press and Circular*.

ABSENCE OF URETHRA—SUCCESSFUL OPERATION.—Dr. Daugherty reports the case in the *Cincinnati Lancet and Observer*.

On the 19th of April, 1868, I was called in haste to visit the child of Jacob and C. Perkins; was informed before leaving my office that it was their first child, a male, three days old; and the difficulty was inability to pass water. On arrival I found

the patient tolerably well developed; very weak; bladder greatly distended; genitals well formed and every way natural, except an entire absence of the urethra—no appearance of the meatus.

Placing the child on its back in my lap, and having its feet and hands secured by assistants, I took a very narrow bistoury, and proceeding to make an incision through the glans penis, tried ineffectually to introduce a very small catheter. I then decided to continue the incision through the body of the penis in the direction of the natural urethra. I proceeded cautiously, stopping at times to try to introduce the catheter. When near the bulbous portion of the urethra but little resistance was offered to the knife. I laid aside the bistoury and succeeded, much to my gratification, in introducing a catheter into the bladder, evacuating its contents. The hæmorrhage was inconsiderable; a few times introducing the catheter succeeded in perfecting a cure, and at this writing (May 20th) the fond parents have the satisfaction of seeing their first-born without maim or defect, a healthy, hearty child.

DEATH FROM CHLOROFORM.—An inquest was lately held at Liverpool on the body of Charles Rollason, forty-six years of age, mate of the ship Countess of Sefton. Deceased being unwell, went to the Northern Hospital, where chloroform was administered by Mr. Frederick Lowndes, in the presence of Drs. Bradley and H. Lowndes.

As soon as the deceased became fully insensible it was noticed that his breathing had stopped. The usual remedies were applied, but without avail. Dr. Bradley stated that the amount of chloroform administered was less than usual, as he had examined the deceased, and suspected that his heart was fatty. A post-mortem examination showed that the deceased died of syncope. The immediate cause of death was the administration of chloroform. Verdict, "died under the influence of chloroform rightly and skillfully administered."—*Brit. Medical Journal*.

DURING the month of June, twenty-eight cases of variola were reported to the Health Officer and sent to the Pest House; of these sixteen have proved fatal, and the remaining twelve are reported convalescent. There has been a little flutter of alarm in the public mind, and re-vaccination has to a very considerable extent been had recourse to.—*California (San Francisco) Med. Gazette*.

Selections and Medical Items.

VIRCHOW'S VIEWS ON SYPHILIS.—He divides the local syphilitic processes into three groups—the simply irritative (inflammatory, hyperplastic), the gummy, and the amyloid; the first two belong actually to syphilis, the last to the syphilitic cachexia. Neither of the first two has added anything foreign to the natural elements of the body—anything in the sense of specific or heterologous, anything peculiar alone to syphilis. Every year, with all its abundance of new material for observation, confirms the author in the opinion that there is no more marked difference between the gummy tumor, with its mass of syphilitic granulations, and a simple inflammatory granulation, than there is between roseola syphilitica and simple roseola. All admit that the same virus is within the body infectious, without contagious. The whole course of syphilis has much in common with that of malignant tumors. It begins, as a rule, with the indurated chancre; this induration corresponds to the mother nodule of the malignant tumor. It may extend in depth and superficially. Then the lymph-glands in the direction of the lymph-current are affected. Then the disease appears in remote points, at first in lighter, simply irritative forms, afterwards in the more severe, and at last in the gummy formations. In these last the induration of the mother nodule is repeated with the modifications imposed upon it by the nature of the local matrix. This is what in all other relations would be designated as metastasis. Whether this occurs through the blood and the lymph, or through cells, is not yet clearly determined. This much is certain, that sooner or later there is an infection of the blood—a dyscrasia is found to exist. Nor is this a permanent state. The idea of latency is to be explained by a metastasis already existing at the period when the original affection was healed, and which only after a certain period makes itself manifest. And so with regard to the tertiary products; if we give up the idea of a permanent dyscrasia, we must either admit a general diathesis of the whole body, a permanent syphilitic condition of all its elements, or else a still existing nidus, a deposit of virus in some given spot; and it is this view that the author does not hesitate to adopt. Thus, after the healing of a chancre and of the symptomatic buboes, a syphilitic hepatitis may continue to exist, without at first developing any symptoms; but upon some casual provocation it may be again excited, may attain increased development, and may become in turn infectious. By infection and the development of a new local nidus the process is made manifest. These views find their application in the history of congenital syphilis.—*American Journal of Medical Sciences.*

MILITARY HOSPITAL ARRANGEMENTS IN PRUSSIA.—Professor Esmarch, of Kiel, has substituted the ordinary shirting triangle for the bandage which Prussian soldiers used to carry in their knapsacks. He has managed the triangle in such a way that the first dressing of wounds and fractures can, behind the firing line, be instantly applied, guns and bayonets being used as temporary

splints. M. Wittmack has sent to the exhibition of Kiel an oil painting representing an action, and the manner in which the triangle should be used. The picture has attracted much attention, and it has been ordered to be printed on each of the triangles given to the troops, so that they may, on the very linen used, see the manner of employing it.—*Medical Press and Circular.*

HOW TO UTILIZE LEECHES.—The German doctors have lately been playing their leeches a droll trick—making one worm do the work of many. When the little blood-sucker has taken his fill and is about to release his bite, he is tapped; a small incision is made in his side, that serves as an outlet for the blood, and he goes on sucking, in happy ignorance of the cause of his abnormal appetite, as long as the doctor pleases. Bellatomy is the name given to the practice, and it is urged that it is not cruel, but contrarywise, since it does the leech a good turn by enabling him to enjoy his rich feast indefinitely. He does not die under the operation, but with proper treatment is soon healed, and may be incised over and over again. There was once an alderman who wished he had been a camel, that he might have been blessed with the seven stomachs vouchsafed by nature to that animal. If such a gourmand still exists, let him seek surgical aid in some such treatment as that practised on the leeches, that he may eat and drink *ad libitum*, and feel no worse.—*Once a Week.*

MEDICAL DIARY OF THE WEEK.

MONDAY, 9, A.M., Massachusetts General Hospital, Med. Clinic. 9, A.M., City Hospital, Ophthalmic Clinic.
TUESDAY, 9, A.M., City Hospital, Medical Clinic; 10, A.M., Medical Lecture. 9 to 11, A.M., Boston Dispensary. 10-11, A.M., Massachusetts Eye and Ear Infirmary.
WEDNESDAY, 10 A.M., Massachusetts General Hospital Surgical Visit. 11 A.M., OPERATIONS.
FRIDAY, 9, A.M., City Hospital, Ophthalmic Clinic; 10, A.M., Surgical Visit; 11, A.M., OPERATIONS. 9 to 11, A.M., Boston Dispensary.
SATURDAY, 10, A.M., Massachusetts General Hospital Surgical Visit; 11, A.M., OPERATIONS.

TO CORRESPONDENTS.—Communications accepted:—Encephalocoele—Intussusception.

PAMPHLETS RECEIVED.—Transactions of the Medical Society of the State of Pennsylvania, at the Nineteenth Annual Session, June, 1868.—Transactions of the Indiana State Medical Society, at its Eighteenth Annual Session, May 19 and 20, 1868.

DEATHS IN BOSTON for the week ending Saturday noon, August 29th, 188. Males, 70—Females, 68.—Accident, 1—aneurysm, 1—inflammation of the bowels, 1—congestion of the brain, 1—bronchitis, 1—cancer, 2—cholera infantum, 47—consumption, 16—convulsions, 4—croup, 1—cyanosis, 1—debility, 3—diarrhoea, 4—diphtheria, 1—dropsy of the brain, 3—drowned, 2—dysentery, 9—erysipelas, 1—typhoid fever, 2—disease of the heart, 4—disease of the kidneys, 1—disease of the liver, 1—inflammation of the lungs, 2—measles, 2—cerebro-spinal meningitis, 1—old age, 3—paralysis, 1—peritonitis, 1—pleurisy, 2—premature birth, 3—puerperal disease, 2—scrofula, 1—disease of the spine, 1—starvation, 1—tumor, 1—unknown, 7—whooping cough, 3.

Under 5 years of age, 87—between 5 and 20 years, 9—between 20 and 40 years, 15—between 40 and 60 years, 16—above 60 years, 12. Born in the United States, 109—Ireland, 11—other places, 11.